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DONDERS ON ASTHENOPIA.

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STRANGE as it may seem, physicians of the present day, as a class, practise better than they preach. The reason of this apparent anomaly is, that modern medical observation and research have discovered certain laws of nature, and certain actions of drugs, which cannot be explained. Practice has consequently outrun theory, and while the physician is content with rapidly gathering in and making use of these results, medical expounders are often delayed by searching after the reason, which they think it their duty to account for, and in doing this they are farther retarded by having, in order to make themselves intelligible, to make use of antiquated terms, which modern science has either rendered inadequate or even meaningless. Since, too, the mechanical and physical means of examining into nature have been carried to such a degree of perfection, it is difficult for a writer to keep pace, through the medium of the press, with the march of science. Before the work appears, some special research has cut off, in their prime, the author's elaborate theories, while an appended foot-note, in the finest print, has to serve, like a graceful epitaph, as an acknowledgment of decease and an appeal for mercy.

On the other hand, this faith on the part of the practitioner is certainly, whatever it may appear at the first sight, a graceful concession on the part of science to nature—an acknowledgment of ignorance, which wisdom in reality alone could afford to make. It is, in fact, a mild triumph of "empiricism" over the once vaunted and scholastic "rationalism," which medical writers and lecturers are usually the last to admit or take advantage of.

That exceptions to the above exist is most undoubtedly true; and among these exceptions one of the most brilliant is Prof. Donders's late work entitled "Accommodation and Refraction of the Eye." This is partly due, perhaps, to the exact nature of the subject, but certainly also, to a great extent, to the manner itself in which the subject is handled. There are, we venture to affirm, few books—

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perhaps no book—on modern medical literature, whose arguments have, on the whole, been more fairly or more forcibly put, or whose propositions have been more clearly and perfectly demonstrated—no book, in fact, whose aim has been higher or whose object more completely attained. And the writer of these remarks can assuredly say that if exceptions have been taken, or questions raised, they have been so without the slightest spirit of detraction, but merely with the feeling that the eminent Professor, in the ardent zeal of newly discovered and newly applied physical and optical laws, has been carried somewhat away from his original intention; so that the very fears which the author himself expressed at the outset, “of depriving his work of the character of exact science,” have been to a degree realized; and, on the other hand, that by dealing at times too exactly with science, he has somewhat lost sight of nature.

From the nature of the subject, the above is more applicable to the chapter on Asthenopia than to the more mathematical portions of the work. It is to this chapter, then, that our remarks will chiefly refer.

In the first place, it has always appeared to us that the use of the word asthenopia, in connection with the condition known as hypermetropia, is inappropriate. The term implies some want of strength in the eye or its appendages. Were asthenopia really, as Donders implies, synonymous with hypermetropia, a hypermetropic eye would then be the one that was deficient in strength. The strength* of an eye is measured by the amount of its range of accommodation, which depends on the muscular force of the apparatus of accommodation, whatever that may be. Now it usually happens that the total range of accommodation in hypermetropic eyes is greater than in emmetropic, because their muscular force, by more constant use and exercise, gains increased strength and activity, with a higher development, as do the other muscles of the body under similar conditions. In this sense, surely, a hypermetropic eye is not an asthenopic eye.

With regard to this, Donders† remarks:—“We should beware of confounding the exciting circumstance with the cause of the affection. The exciting circumstance of the phenomena consists in continued tension in looking at near objects; the cause, on the contrary, is the hypermetropic structure of the eye. In fact, asthenopia is not the fatigue itself, but the want of power through which the fatigue occurs.” The saying that asthenopia is the want of power through which the fatigue occurs, and that the hypermetropic structure of the eye is the cause of the fatigue, is simply saying that asthenopia is the same thing as hypermetropia, or, in other words, that a hypermetropic eye is one “which wants power.” Now we can hardly call

* In considering the strength of an eye, no reference has been made to the acuteness of vision, as amblyopia is not necessarily a factor of asthenopia or even of hypermetropia.

† Accommodation and Refraction of the Eye (Donders), p. 261.

an eye which has greater muscular force than a normal eye, while its vision is equally acute, wanting in power. It seems to us exactly the reverse, namely, that *asthenopia* is the fatigue itself, and not "the want of power." For example, a weak man may carry a light load a certain distance with a certain amount of fatigue, while a strong man carries three times as much, but with the same amount of fatigue. The result is the same in both cases, but the causes were exactly the opposite. In the first case it was a deficiency of strength, but in the second an excess of weight; and nobody would call the strong man a weak one simply because he was fatigued by carrying too heavy a burden. Just so a hypermetropic eye is not fatigued because it is wanting in power, but because, though stronger than normal, it is overburdened. Hence the inappropriateness of the term as applied to hypermetropia.

To a certain extent Donders is not responsible for the word *asthenopia*, since it existed before he wrote, though it never did in the sense in which he uses it. It was used by former practitioners as one of those convenient cloaks, so often employed in medicine—a garment, by the way, still in fashion—to cover what their intellect could neither understand nor explain. It was applied to a certain set of symptoms whose causes could not be fathomed. It was to ophthalmic surgery, what the terms jaundice and dropsy were formerly to general practice.

Donders brought forth and elucidated a very frequently occurring cause of the set of symptoms denominated *asthenopia*, and immediately applied the general, vague and comprehensive term to this particular cause. Had he, now, insisted that the preëxisting term should be given to this individual cause, and had he, by the very weight of his authority (which he might have done), made the profession adopt it, he would then have been justified in making a part stand for the whole, and in thus making hypermetropia synonymous with *asthenopia*; and that he really does so consider them would without doubt be inferred from the following observations:—

"I have already asserted that hypermetropia is usually at the bottom of *asthenopia*. The truth of this assertion has been doubted. I now, however, go a step further, and venture to maintain that, in the pure form of *asthenopia*, hypermetropia is scarcely ever wanting." (P. 261.)

"Our knowledge had reached this point when I discovered the cause of *asthenopia* in the hypermetropic structure of the eye." (P. 274.)

"It is a great satisfaction to be able to say that *asthenopia* need now no longer be an inconvenience to any one. . . . The discovery of the simple fact, that *asthenopia* is dependent on the hypermetropic structure of the eye, pointed out the way in which it was to be obviated."

The above statements would seem to imply, with some force, that hypermetropia was either invariably the cause, or so nearly so, that its occurrence from other causes and conditions were not worth mentioning; and it would most assuredly convey such an impression to the reader's mind, did he not happen to remember that the author

had previously stated,* that "the condition for the occurrence of asthenopia may be still more generally formulized; it is the presence of a rather considerable, but at the same time insufficient range of accommodation. Now, in general, this insufficiency is attributable to hypermetropia, as has been fully explained. But it may proceed also from want of energy, and in paresis; insufficiency of the interni recti muscles, and in astigmatism." These four other causes are somewhat contradictory of "the simple fact that asthenopia is dependent on hypermetropia."

On page 274, the author blames Stellwag von Carion for implying that he considered asthenopia to be a lesion of the accommodation, while on page 264 he admits that it may be due to paresis; and again, on page 277, he strengthens this admission by observing, "in the establishment of the rules to which experience led me, I must distinguish between two series of cases—*a*, those which, with normal range of the accommodation, are exclusively dependent on hypermetropia; *b*, those where diminution of the range of accommodation, or want of energy, plays a more or less important part. The cases contained in it are characterized by the fact, that accommodation itself is disturbed, or morbidly diminished." What is the difference between saying, that sometimes the accommodation is "itself disturbed," and implying that asthenopia may be a "lesion of the accommodation"? Is it any worse to make asthenopia almost invariably the result of a lesion of the accommodation, than to make it almost invariably the result of hypermetropia?

On page 289, the author, under a caution against mistaking apparent for true asthenopia, describes, in his masterly way, a case of what, in this country at least, would be called a typical case of asthenopia. But as it is connected with a slight degree of myopia, instead of hypermetropia, it is denominated apparent, instead of true asthenopia. With regard to the treatment in this case, where the myopia equals only 1.36th, $V. = 1$, and $A.$ good, Donders observes that "blue glasses, resting the eyes, stimulating derivatives, are only too often tried in vain"; a remark which is calculated to somewhat subdue the encouraging effect of the previously made assertion that "asthenopia need now no longer be an inconvenience to any one," especially as the author admits that "such cases are not uncommon. They occur mostly with myopia, but they are also met with in other eyes."

This is simply saying that asthenopia need no longer be an inconvenience to any one, provided that it comes from hypermetropia. But provided that it does not proceed from hypermetropia, the inconvenience may be so great as to defy all therapeutic agents; and the statement that asthenopia is almost invariably connected with

* Page 263.

hypermetropia, does not appear compatible with the one that such cases are not uncommon and are to be met with in myopia.

Such reasoning as the above, we maintain, is begging the question, and obscures rather than enlightens the subject.

Invaluable as are the researches of Donders with regard to hypermetropia, they would have been more so had he insisted upon bestowing upon the symptoms, coming from a definite condition, some definite and appropriate name, and not included those cases coming from an anatomical defect with those arising from functional derangement. Had he done so, he would have eliminated at a stroke a frequently occurring and very important factor of asthenopia, and narrowed, to a great extent, the borders of that dread region which before his labors was almost boundless.

But is it true, as Donders affirms, that the great majority of the cases of asthenopia are contained in his first category, viz., "those where the cause is exclusively the hypermetropic condition of the eye"? We think that future experience will prove that this is not the case, and for the following reasons:—

1. Because asthenopic troubles are found most frequently among the upper classes, and it is precisely here that hypermetropia, like all other incomplete development, occurs the *least* frequently.

2. Because asthenopia is oftener met with in myopic than in emmetropic eyes, and myopia is a condition which belongs essentially to the upper classes; and the causes which increase this condition, when once formed, are just those, viz., continued application of the eyes on near objects, which would rationally produce asthenopic symptoms, for an overfatigued eye (even if it be emmetropic) is in fact an asthenopic eye, just as any other overfatigued organ is a weakened organ.

3. For the reason that in slight cases of hypermetropia, where the asthenopic symptoms are entirely disproportionate to the amount of hypermetropia, patients, when supplied with the requisite glasses, often discard them after a time and experience no further trouble. This is especially the case when such hypermetropes are suffering under some temporary trouble which reduces their general health. I have seen many such cases, from two of which the following brief extracts are taken:—

Miss B., æt. 25, in 1860 had sudden and violent pain in both eyes after prolonged application of them on bad print. The seat of this pain was in the *ball* of the eye and not over the forehead. She was kept in a dark room for several weeks. Since that time the patient has had no return of violent pain, but latterly has complained of a dull, aching pain on use, which is now *above* the brow as well as in the eye; letters run together, and the patient has in fact all the symptoms of hypermetropia. There is, moreover, slight hyperæmia of the conjunctiva in both eyes. The movements of the eye are perfectly normal and the interni muscles strong. Hm. = 1-30th. Ht. = 1-24th. V. = 1. A. = 1-5th. Fundus normal. General health much below par, and she is pale and cachectic. Tonic treatment was prescribed, and + 1-30th, with which the patient was told to read three times a day, increasing gradually, in accordance with Dr. Dyer's method. Under

this treatment the patient progressed but slowly for three weeks, at the end of which time she could only read without pain fifteen minutes at a time. Discouraged with her slow progress with the glasses, she wrote two letters without them, and was then attacked by a violent pain in and about the eyes, which lasted for forty-eight hours. She returned to her glasses. Six weeks later, she could only read thirty minutes three times a day. She continued to gain slowly, when she had a violent attack of inflammatory rheumatism, which confined her to the house for more than a month, and she passed out of my care. Four or five months afterwards the patient, whose general health seemed to be nearly restored, visited me again. She informed me that her eyes gave her no further trouble, that she read and wrote as much as she pleased, and that she had not used her glasses since her recovery from illness.

A. C., æt. 15. Hm. = 1-40th. Ht. = 1-24th. V. = 1. A. = 1-4th. Fundus normal. With this patient letters did not become indistinct, but vision could not be maintained for the near, without inconvenience, more than a few minutes. No weakness of the interni could be detected, though the attempt was made several times. The same treatment as in the preceding case was adopted for several weeks, when the patient was allowed to resume his studies, with the injunction never to use his eyes for any close work without his glasses. Six months later I saw the patient, when he assured me that his eyes were *perfectly well*, that he had not used his glasses for the last four months, although he had been in the habit of using his eyes till a late hour of the night in preparing his college exercises.

If this can happen, as it often does, where the total hypermetropia equals 1-20th to 1-24th, how much more frequently would it be likely to occur in the slighter degrees of hypermetropia?

The above examples of frequently occurring cases are not quoted here to prove that hypermetropia had nothing to do with the asthenopic symptoms, but merely to show that it was the secondary and not the immediate cause of the trouble; for if it had been the primary cause the asthenopia would, on the one hand, have disappeared the moment the hypermetropia had been neutralized by the glasses, while, on the other, it would have returned as soon as the glasses had been laid aside. We think, therefore, that practitioners have, heretofore, been too prone in cases where trifling degrees of hypermetropia have been detected (say of 1-40th to 1-70th) to attribute the asthenopia which occurs in such eyes *invariably* to hypermetropia, and that they have been thus led to class these cases under it. Further, we have no right to assume that the slight degrees of hypermetropia brought forth by paralization with atropine are due to a want of length of the antero-posterior axis; because, it is by no means proved that atropine simply paralyzes the asthenopia. We do not know but that in paralyzing one set of fibres it does not cause others to contract, and thus cause the lens to become a trifle flatter than it was before the application. Atropine certainly produces a wider dilatation of the pupil than that which takes place in total paralysis, or even in removal of the oculo-motor nerve. May it not produce the same effect upon the fibres of the ciliary muscle that it does on those of the iris? It causes the emmetropic eye, on the average, to become hypermetropic 1-60th,* and this effect may differ greatly in different individuals, especially in the young. And would it not be far fetched

* Accommodation and Refraction of the Eye, page 600.

to attribute grave asthenopic symptoms, as is often done, to this amount of hypermetropia? The fact that some people, who are slightly hypermetropic, cannot use their eyes without the aid of a weak convex glass, does not prove that their asthenopia was produced by the existing hypermetropia. This we hope to elucidate further.

Finally, as strongly corroborative of the assertion that the majority of asthenopic troubles are not due exclusively to hypermetropia, we would call attention to the statistics published in the "Transactions of the American Ophthalmological Society," 1865, by Dr. Derby, who says:—"I have carefully collated the cases of asthenopia occurring in my own private practice, excluding infirmity patients as offering relatively fewer cases of the affection, and less opportunity for subsequent study. Out of 1800 recorded cases in general ophthalmic practice, I find 369 cases of asthenopia in general. Of these 241 belonged to the first class and 125 to the second, or asthenopia simplex. Nearly all of these latter were dependent upon hypermetropia." It would appear, according to this, that the cases which were dependent upon hypermetropia were less than one to two of those where the asthenopia was due to other causes. This disproportion would be greater still did not the above statistics embrace also those cases dependent on insufficiency of the interni recti; while many cases must have been included also where the hypermetropia was of a very trifling degree. My own cases, though not so numerous, give nearly the same ratio.

It was originally proposed to divide asthenopic troubles into two classes:—

"1st. Where the continued use of the eyes on near objects is physically—

"2d. Where it is optically impossible."

This classification has lately been given up for one instituted by Donders, who, maintaining that the hypermetropia was almost invariably the cause, insisted that this condition should represent the type of the affection.

A speedy return to the former classification (which at least had the merit of covering the whole ground) would certainly appear most advisable.

The practical objection against the present classification is, firstly, that it makes what are in reality the exceptional cases predominate over the typical ones; secondly, that it tends, especially when upheld by such weighty authority, to stop scientific investigation, and makes us content with a part of the truth when by patient research we might obtain the whole.

One of the greatest obstacles to the progress of medicine has always been its imperfect and meaningless nosology, and no branch has certainly suffered more in this respect than ophthalmology. To remedy this will be assuredly a herculean task, but it is high time that it should be begun.

DR. VALERJ'S INTRODUCTORY LECTURE.

(Continued from page 337.)

BUT not only the natural course of diseases teaches us that they are cured by the crises, but also that these crises do not act irregularly or confusedly, nor in a uniform manner in all kinds of maladies. This standard fact of the crises is comprehended within the directing law of all created things, as I have already stated in speaking of the conservative nature of our economy in its healthy state; that is to say, even the crises are accomplished *cum ordine, tempore, et mensura*.

Orderly.—You do not find, for instance, that an indigestion is cured by perspiration, but by vomiting, and conversely a rheumatism is cured by perspiration, not by vomiting; an abundant flow of urine will prove critical for a dropsy, and a dropsy will not be cured by a puriform expectoration, as is the case with bronchitis or pneumonia. An epistaxis will decide favorably the issue of a congestion, an inflammation or a fever, but not of smallpox, for instance, nor scarlatina; this latter is arrested by a desquamation, and the Arabic exanthema by suppuration. And for briefness's sake, I will state, that every morbose process has a particular crisis relative to the nature of its cause, the tissue and organ that it attacks, the course it pursues, the age, constitution and temperament of the individual, epidemic influence, &c. We may lay it down as a general rule, that the crises for hæmorrhage ordinarily appertain to inflammations, those for mucous secretions to phlogosis of the mucous membranes, so widely extended over such a number of apparatus; the crises through the skin to a great variety of pyrexia of a nervous and humoral character; in fine, that the critical fluxes of the glandular system belong rather to malignant and typhus fevers, the plague, &c. &c.

Within due limits.—The reason of this is so clear, so intimately connected with the very nature of the crises, that it requires but a brief explanation. If the critical perturbations were too violent or too gentle, if the salutary evacuations were too abundant or too scarce, in other words, if they were not commensurate with the nature and specialty of the manifold pathologic processes which they are to resolve, they would be no longer beneficial, and so far from effecting a perfect cure, would either prove useless or detrimental to the affected organism. *Nil paucum criticum* essentially comprehends the inverse aphorism of *nil multum criticum*. Thus, for instance, a hæmorrhage, a fit of vomiting, a perspiration, a diarrhoea, if irresistible and ungovernable, so far from being completely favorable to a congestion, a gastric obstruction or a fever, would reduce the patient to a state of deplorable inanition: on the other hand, we do not find that a slight purulent spitting empties a vomica, nor that a few drops of blood are sufficient for a real plethora, nor does a diuresis of a few hours dissolve a general anasarca, and so on with regard to

analogous cases. The crises, then, exercise themselves within sufficiently just limits to operate a perfectly favorable change in diseases, which is the scope of the salutary nature. If at times this does not occur, there are reasons for it, which I shall explain to you hereafter. For the present you will permit me, for the due order of my discourse, to speak only of the healthful and benignant side of nature, or of the operations she performs purely, and without any concurrence of ours, for the welfare of our affected organism.

At proper periods.—Although by the simplest notions of physiology, which plainly teach us that the whole economy, as regards increase or decrease, action or repose, as well of itself as of all its tissues, organs and functions, is influenced by time's silent sway; although by the simplest notions of pathology, which teach us that each peculiar malady takes its proper course almost invariably, physicians should, even previous to their individual experience, have unanimously foreknown and concluded that even crises are regulated by time, that is to say, by a law which appoints the periods of their first appearance and of their maturity; notwithstanding all this there has been no truth in medicine so much discussed, contested, and, I would also say, wrongly interpreted, as that of critical days. Sydenham, who next to Hippocrates may be justly considered as the most experienced observer of diseases, surprised at the *regularity* of of their *periods*, the peculiar order and method with which they occur, and considering the phenomenon neither more nor less surprising and mysterious than so many other realities which occur in creation at appointed periods, rather than confute the objections brought forward by the opponents of this doctrine, meets them with the following query:—"Tell me, explain me, I pray you, if you make so great account of yourselves, why the horse attains to its perfect growth at the age of seven, man at the age of twenty-one; why this plant is wont to flower in May, that in June, another in a different month; and so on with so many like phenomena of the Creation?"

Libenter enim a vobis questierim cur æquus ad acmen suum pertingat VII. annorum spatio, homo XXI., cur in plantarum regno hæc mente Majo, illa Junis, ista alia florere soleat, ut innumera ali nunc sileam?

As for us, we shall deport ourselves in a like manner, preferring, like Sydenham, not to discuss the arguments brought forward against the existence of critical days; but simply noting the fact, we declare that, despite the opposition from so many quarters, the doctrine of the critical days has never fallen to nought, but still lives and prospers. Philosophers and physicians, even the very vulgar, and patients themselves, all respect it. "*Savans et peuple, medecins et malades tous admittent la doctrine des jours critiques dans les malades.*"—Ballonius, Epid., lib. 1st. In fact, observation, the most faithful guide to our attainments, teaches us that as diseases have their regular periods of increase, acme and diminution, so their great and manifest changes, favorable or unfavorable (*ad salutem aut mortem, ad melius aut ad pejus*

inclinationes), mostly take place on certain stated days, called therefore *decretory*, while on certain others they are pre-announced, and these days are hence called *indicatory*; and on certain others they occur but rarely, or in an irregular, imperfect, and often fatal manner, and these last are called *intercalary* days. I shall not here stop to specify the cyphers which represent these different days; you have certainly engraven them in your memories. However, there is one thing which I deem it not superfluous, but even useful to be remembered, and it is that Hippocrates was the first who established and noted their importance. Nor in doing this was his mind pre-occupied by any pre-conceived numerical law; but endowed with a sublime genius, which no law affecting the patient could escape, by merely observing with clear introspection the natural progress of the morbose processes, he distinctly perceived that their issue in recovery or death, and the critical phenomena which influenced them beneficially or otherwise, for the most part occurred on stated days, which, however, he does not lay down as unexceptional, but the most oft-recurring, not as regards the universality of diseases, but the greatest number of them, not as absolute, but dependent upon a multitude of circumstances; as, for instance, the diversity of age and strength of the patient, their surroundings, difference of seasons and climate, epidemic influences, &c. It suffices to read the golden work by *Testa*, a celebrated physician of Ferrara, "*De vitalibus periodis aegrotantium et sanorum*," and Anthony de Staen, in his "*Ratio Medendi*" (wherein, with unmatched erudition, he analyses the course, crises, and critical days of more than two hundred cases of maladies and epidemics recorded in the works of Hippocrates), to be convinced that this truth rests on unshakable evidence, connected with which it will be of some service to repeat the fact, that the father of medicine, in enumerating the critical days, had no other guide than mere observation. The result, then, is that physicians of every age and country, independent of every system, and true to the maxim of observing and never tampering with the operations of nature, whenever she was self-sufficient to cure diseases, unanimously noticed not only the crises, but the regularity of the critical days. *Foresto*, for instance, gives the history of forty-eight cases of intense malignant fever, thirty-eight of which terminated on the critical days, declaring with rare minuteness, that among these patients five were favorably determined on the fourth day, twenty-two on the seventh, seven on the fourteenth, two on the eleventh, one on the seventeenth, and another on the 21st. Now, if out of forty-eight patients three fourths were determined favorably on the critical days, these days must be distinguished from the others; and if out of these critical days twenty-two were favorably decided on the seventh, and seven on the fourteenth, out of the sum total of thirty-eight cases, there is no doubt that these numbers seven and fourteen are to be preferred to the others. Jandie Bauvais, in his "*Simeiotique*," states as follows:—

"I can positively assert that, during my twenty years' experience, I have always observed the crises occur at the periods noticed by Hippocrates, in all cases wherein no disturbing medicine interfered with the regular course of the disease; and my pupils at the *Salpêtrière* have frequently seen the crises occur on the critical days even among the aged." Andral himself, in his medical clinique on acute diseases of the chest, has observed that out of ninety-three cases of recovery, three fourths of them coincided with the critical days.

But the critical evacuations on critical days are not the sole means by which nature healeth: another mode of recovery, equally efficacious, and confirmed by daily experience, is *metastasis*, that is the changing of the seat and form of the disease, by means of which the grave symptoms of an affection, in most cases internal and dangerous, are arrested, giving place to the manifestation of another affection of much less importance, and often curable. This fact, notwithstanding the different interpretations which it has received, in conformity with the various systems which have prevailed in medicine, is incontestable, and a few examples will suffice to convince you of its truth.

Suppose that a certain person has suffered for many years, from a persistent dyspepsia, or if you will, from vertigo, or neuralgia in any region whatsoever, or from a violent intestinal colic; and suppose that (no matter which of these morbose forms) it attacks him with recurring paroxysms several times a year, and at each time with redoubled intensity. All on a sudden he is fortunately attacked by a fit of gout, and from that instant the vertigo, the dyspepsia, the colic, or any other serious malady whatsoever that a shifting and visceral gout can engender, ceases, never more to return. A girl is affected from hemoptysis, which after recurring several times, is finally accompanied by slow fever with cough, pain in the chest, puriform sputum, and a certain degree of general consumption. The case is adjudged to be most serious; but while the members of the family almost despair of her recovery, there appears on the skin of the patient a herpes inherited from her parents; the pulmonary syndromes are checked, get better, and finally cease completely, and the poor girl recovers henceforth her strength and health. A man of bilious temperament, but of a robust constitution, in a few years becomes quite indisposed, at first by a fierce hepatitis, then by jaundice of long duration, and ultimately by repeated attacks of cerebral congestion. On a fine morning he ejects with his excrements a copious quantity of black venous blood, preceded for several days by more or less tenesmus, costiveness, and a sense of burning along the rectum: from that moment sets in a hemorrhoidal and periodical flux, from which a long series of years of perfect health compensates him for his by-gone dangers and sufferings. I should draw out this most important subject to a great length if I had to enumerate to you the many like instances, which are daily beheld in medical practice;

the above cases, cited as an abstract, are the most common, and tested by my personal experience. It is not unfrequent, for example, to see a pleurisy, or pneumonitis, or a meningitis suddenly resolve itself by the appearance of an erysipelas; a persistent convulsive cough arrested by means of an eczema, a *fluxus post aures*, or by an acne appearing on the skin of children; an urticaria cure a cardialgia, which for so many years baffled a number of remedies; the appearance of a fetid perspiration on the feet cure a persistent laryngeal cough in a boy threatened with pulmonary tubercles, and from being thin, and delicate, render him in a short time florid and robust. I myself have seen a spontaneous eruption of boils heal a melœna, which threatened speedily to carry off a noble personage; and not long since I have witnessed a hemorrhoidal affection which permanently arrested a copious hematuria recurring at very brief intervals. In perusing the clinical histories by authors of sound practice, you will find an ample collection of such like recoveries from maladies reputed even incurable, and ultimately brought to a favorable issue by nature in this manner, namely, converting them into diseases of much less importance and danger, by transferring at once the morbose matter or process lodged in a viscus to an external, or less important part of the body. I cannot, in the meantime, too strongly impress upon you not to forget this salutary procedure—or the practical importance derived from it in effecting the diagnosis, prognosis and cure of many diseases we shall treat at the sick bed; suffice it for the present for you to know that so great is the benefit which nature designs to bestow on the patient by this means, that when, like the exclusive anatomo-pathologists you would suppress or inopportunately cure these metastatic manifestations, you inflict the forfeit of death on your poor patients.

But nature, as if not content with curing diseases by means of the crises and metastasis, is wont, moreover, to excite, by a mysterious influence in the affected organism, certain appetites, and sensations, in being blindly obedient to which, the individual, by satisfying them, finds the only hope of his recovery. I have already addressed you on the subject of these internal spontaneous motions and stimulants, otherwise termed instinctive desires, and stated to you that they not only contribute towards the maintenance of health, but moreover, in several cases towards the cure of maladies. Let us take, for example, a person affected with dropsy, whom the copious variety of diuretic medicines has failed to cure: reduced to the last extremity, he feels a rabid yearning for a certain quality of wine; it is given to him, perhaps against the physician's will; he drinks it, and is cured through the intermediation of a critical diuresis. A person threatened with consumption, whose diarrhœa is increased even by the most selected aliments, feels an unwonted craving for oysters, and by partaking of them the gastric flux becomes moderated, ceases, and he happily recovers his health. A patient affected with a severe dysen-

tery recovers by eating to satiety and with voracity certain pickled meats. Some aged person, ill treated, with a quartan ague, is cured by plunging into a cold bath, to which he felt himself impelled in the height of his feverish paroxysm. How many chlorotic females, whom tonics and ferruginous remedies have failed to invigorate, by merely satisfying their extravagant appetites for hard food and indigestible substances have restored the crisis of their impoverished blood, and thus re-acquired the regularity of their catamenia, to the astonishment of themselves and their physicians! Nor shall I dwell further on these facts, so strikingly curious, interesting and mysterious, for the history of medicine supplies us with them most abundantly; and you yourselves, at your leisure, may make an ample collection of them.

We have, then, heretofore seen that nature cureth diseases, and passed in review the means and order which she adopts for this purpose. I would not, however, that any of you should deem yourselves adequate to solve, by these sole elements, the question expounded in the opening of our discourse, and which I characterized as not being very easily judged with impartiality and soundness of discernment. In fact, if astonished at the extraordinary power which nature displays in the cure of diseases, you decide the cause by a *simple* and *hasty* sentence in her favor, and against the physician, you would expose yourself to two strong objections, which would not fail to present themselves to you, and would certainly prove very embarrassing. And these objections are, *firstly*, that nature by curing and operating so marvellously, seems to act with discernment and intelligence. *Secondly*, that for this reason she may be sufficient of herself alone to cure diseases, and, therefore, the *ars medica* and the physician are superfluous, if not hurtful.

[To be continued.]

Reports of Medical Societies.

EXTRACTS FROM THE RECORDS OF THE NORFOLK DISTRICT MEDICAL SOCIETY OF MASSACHUSETTS. BY Z. B. ADAMS, M.D., OF ROXBURY, ASSIST. SECRETARY.

A STATED quarterly meeting of the Norfolk District Medical Society was held at Dedham, Nov. 14th, 1866, at 11, A.M. The President, Dr. Cotting, in the chair. The records of the last meeting were read by the Secretary, Dr. Jarvis, and accepted.

Dr. P. O. M. Edson, of Roxbury, having been examined by the Censors, was admitted a member of the Society.

The President said that he was happy to inform the Society that twelve or thirteen gentlemen were ready to read papers.

Voted, on motion of Dr. Faulkner, of Jamaica Plain, that the discussion assigned for this meeting precede the reading of the papers.

Voted, on motion of Dr. Jarvis, of Dorchester, that a committee be

appointed by the Chair to select a subject for discussion at the next meeting.

Drs. Howe of South Weymouth, Miller of Dorchester, and Monroe of Medway, were appointed. They subsequently reported as follows: "Are the constitutions of our women degenerating? If so, what is the cause?"

The President announced the subject for present discussion to be—"The Effect of the Present Method of Common School Education on the Mental and Physical Development of the Child."

Dr. Green, of Milton (one of the appointed disputants), opened the discussion. He said that there were one or two points, which were defects, to which he would call attention. He spoke of pushing scholars up from the lower to the higher grades too rapidly. He thought committees were responsible. The high school course should be looked upon as supplementary. A strict written examination being now required for entrance to the high school, the grammar school teaching was but a system of cramming for this. Committees pay too little attention to the lower grades, and all new or inexperienced teachers were put in charge of the primary schools. He suggested that some other system of examination for entrance to the high school than this written examination should be had.

Dr. Seaverns, of Roxbury, remarked that he thought that the members of the Society ought to use their personal influence to remedy the defects pointed out; but he also thought that ventilation and other defects spoken of by Dr. Salisbury, in his address last May, had not yet been remedied. School-rooms were too warm, close, and ill ventilated. He referred to the schools that he had visited. Also the studying of lessons out of school should be abolished.

Dr. Fifield, of Harrison Square, objected to the mode of seating the children at school on chairs all alike, fastened at the same distance from the desk, compelling the children to sit immovable. He also objected to forcing all children along in the same stereotyped course of studies.

Dr. Faulkner, of Jamaica Plain, said he had no practical knowledge of the subject, but sympathized with the opinion of one of our seniors, who had expressed grave doubts as to whether the method adopted when he was a boy, when no attention was paid to the subject, was not better than now, with all the improvements.

Dr. Howe, of South Weymouth, said he thought school sessions were too long, and children were sent too early. He thought study should be required only a portion of the year.

Dr. Munroe, of Medway, said he thought there was a neglect of the physical powers, while the mental powers were strained to the utmost. We did not begin early enough. The child should be educated from the cradle; the systematic performance of duty should be the aim. The necessity of labor, or work, should be taught from the earliest infancy. He believed that the morning session might be devoted to some useful labor, and that the afternoon would be enough to acquire all that is now done.

The Secretary, Dr. Jarvis, of Dorchester, said the question was, How shall these children get the greatest amount of power for work? not how shall they learn Latin, &c., but whether, while learning such things, they got the greatest power. As a general rule, our child-

ren go out as well prepared mentally and physically as could be expected. Ventilation is much better now than it used to be. He thought well of the half-hour system, alternating study and work, which had been introduced in England. He also spoke of the necessity of properly adjusting the bodies of the children in school.

Dr. Noyes, of Needham, was glad to have the profession here express their opinion upon this subject publicly and with authority. The brain was too much taxed, the body too little. The gymnastic system did not answer the purpose; exercise should be such as to develop the whole body. By the present system the brain was too much exercised, the muscles not enough. The system should be to encourage and foster, and draw out the talents of each individual.

Dr. Mann, of Roxbury, said he objected to withdrawing children from the public schools and sending them to private schools. We should try to remedy the defects of public schools, and it is a duty we owe to the community to make the effort.

The President, Dr. Cotting, summed up the discussion. There were evils, certainly, still needing remedy. The necessity of utilizing the knowledge imparted at school was too much overlooked. Children are drawn, or forced, through forms and systems, literally "crammed," without being sufficiently taught how to use what they learn in further acquisitions and in the ordinary occupations of life. There is a general impression, not wholly without foundation, that our schools do not answer their purpose as fully as they should. There was too much "education" and too little real available learning.

Dr. Stone, senior, of Walpole, moved that papers be now read. Agreed to.

Dr. Adams read a review of Vol. II. of Niemeyer's work, "*Elements de Pathologie Interne et de Therapeutique.*" On file for publication.

Dr. Arnold, of Roxbury, read a paper on the Employment of Hydrostatic Pressure in Nebulizing Fluids for Local Anæsthesia, showing an apparatus devised by himself. On file for publication.

The President said that Dr. Arnold had used the apparatus for him most satisfactorily. Not long since, Dr. Arnold thus assisted him in some cases of "ingrowing toe-nail," a disease which he alluded to at this time because Annandale, in his recent large work on the Surgical Treatment of the Toes,* re-printed the present year in this country, says, page 105, that evulsion of the nail, "the plan proposed by Dupuytren, is certainly the best." He, the President, had never found it necessary to remove any of the nail; on the contrary, in one of the cases in which he had had the assistance of Dr. Arnold, the nail had been removed some years before without any good effect. Indeed, the failure of removal of the nail induced him originally to adopt the present plan. His method, as formerly described to the Society (*Boston Medical and Surgical Journal*, vol. lxxiv., p. 340), is to remove a portion of the sound as well as diseased flesh from the side of the toe, say three quarters of an inch long, half an inch wide, and as thick as the member will admit of. Two cases, so operated on since the last meeting, succeeded perfectly.

* Malformations, Diseases and Injuries of the Fingers and Toes, and their Surgical Treatment. By Thomas Annandale, F.R.C.S., Edin. 8vo. Philadelphia. 1866.

Dr. Stedman, of Dorchester, stated that had recently performed the operation as above described, with complete success.

Dr. Miller, of Dorchester, read a paper on a new principle of treatment in uterine surgery—intra-uterine scarification. This paper was eminently practical and instructive, and many asked for its publication. On file for that purpose.

Dr. Faulkner inquired if the operation prevented conception and healthy parturition.

Dr. Miller replied that, on the contrary, it was extremely useful as a remedy for sterility, and to prevent the discharge of the fœtus before full term.

He also showed a specimen of amputation, performed on his way to the meeting, of the neck of the uterus, which protruded through the external organs. Case of fibrous degeneration. Removed by the *ecraseur*. The portion removed was about two inches long.

Dr. Campbell, of Roxbury, read a paper (see last number of this JOURNAL, p. 346) describing splints made of pasteboard strengthened by hoop-iron, very cheap, light, strong, and easily made on the spot, and related a case of fracture of both bones of the leg of an epileptic subject, treated by this splint with excellent success.

The President said, that in suggesting this splint to Dr. Campbell, he claimed nothing original, but that Dr. Allen, of Roxbury, had, years ago, made a similar splint for him.

Dr. Cotting also stated that he had now a patient with fractured patella, to whom he had applied a splint made as above described, with modifications suited to the case. The cost of this, besides what the house supplied, was fifteen cents, and the same would probably cost half as many dollars if made in the ordinary way at the manufacturer's. He also said that the actual value of splints in the successful treatment of fractures had probably been over-estimated both by the public and the profession—that shortening and deformity were less controlled by them than generally supposed; hence often much unnecessary suffering inflicted upon patients, and much unfounded dissatisfaction in the patients and friends at the result. He suggested that the true value of splints in the treatment of fractures would be a good subject for discussion in the Society, or for a paper.

Dr. Noyes related a case of fracture of the arm, treated by a sole-leather splint, made on the spot.

Dr. Edson spoke of Smith's anterior splint, particularly useful in compound fracture of the thigh from gun-shot.

Dr. Stone, junior, of Walpole, related a case of compound fracture of the left femur and right foot by railroad accident; and showed the patient. Treated by extension, and without splints. The result was satisfactory. Notes on file.

Dr. Faulkner read a report of a case, merely, he said, with the design of asking what ailed the patient. The case was marked by diarrhœa, thin and watery, without pain, lasting for several days, as often as once an hour. Pulse 88. Skin healthy. Thirst urgent. Vomiting came on, and stools continued, accompanied by anguish and great depression or sinking; in paroxysms of which he wanted to have his arms pulled. Face became cold. Cried for ice. Wanted doors and windows opened. Vomiting ceased after about twenty times in all. Surface not hot or feverish. Vomiting and diarrhœa stopped thirty-six

to forty-eight hours before death. Mind unaffected. *Post-mortem* appearances.—Great cadaveric rigidity. Body warmer than before death. Stomach healthy. Heart filled with black blood. Very little blood in the tissues. Intestines were denuded of epithelium, and full of thin yellowish serum.

Several members expressed belief that the case was one of cholera. At this point, as bearing upon the case, Dr. Cotting read a paper on Cholera in Roxbury and Vicinity, during the past season. There had been about twenty deaths, and an equal or greater number of recoveries.

Dr. Fifield read a paper—A Critical Review of the Surgery in Rankin's Abstract and Braithwaite's Retrospect—a learned paper, ending with an amusing classical comparison between the two compilations.

Dr. Stone, senior, moved that, on account of the lateness of the hour, the other papers be deferred to the next meeting.

At quarter to 4 o'clock, P.M., after one of the fullest and most animated sessions it ever held, the Society adjourned.

THE BOSTON MEDICAL AND SURGICAL JOURNAL.

BOSTON: THURSDAY, NOVEMBER 29, 1866.

VIVISECTION.

THE practice of vivisection, of conducting physiological experiments on living animals, of laying bare the organs within the body and demonstrating to the eye the vital processes and changes which otherwise must be left very much to conjecture and theory, is one which, considered purely in itself, naturally excites the strongest repugnance in every mind which lays the slightest claim to humanity. The sight of the suffering thus inflicted on the lower animals cannot but cause a shudder, and it requires a conscientious purpose on the part of the operator and an intelligent conviction on the part of the spectator that an investigation is being made the object of which is the physical welfare of a higher race, to justify it or to make it even tolerable. Doubtless there has been, in times past, too much reckless experimentation of this kind, too frequent repetition of the same experiment, too great readiness for mere purposes of display to exhibit what has already been sufficiently demonstrated, and too little consideration for the suffering thus unnecessarily produced. As we have said, it requires a full appreciation of the importance of the object sought for to make such investigations tolerable. It is not, therefore, a difficult thing to so describe them, particularly to persons not professionally interested, as to make them to their minds the very consummation of everything barbarous and cruel. This view may, nevertheless, be as unwise and unjust as the criminations of those who stigmatize the operations of the noble science of surgery as nothing better than butchery.

Influenced by such feelings, it is not surprising that movements should have been made at different times on the part of benevolent

persons to check an undue tendency to the repetition of such experiments, or even to prevent them altogether by legal enactment. In this country, until quite recently, very little of this method of pursuing physiological study has been done. Professor Dalton, of the College of Physicians and Surgeons in New York, has probably made more researches in this way than any other American physiologist. The value of the results which he has obtained has been universally acknowledged by the medical profession; and as it is every day becoming more and more evident that the true practice of medicine must be based on the most accurate physiological knowledge, the value of the information gained in this way is becoming more and more demonstrated. Prof. Dalton's vivisections, however, have attracted the attention of the New York Society for the Prevention of Cruelty to Animals, and its President, Mr. Henry Bergh, has felt it his duty to address a letter of inquiry on the subject to Dr. Delafield, President of the College of Physicians and Surgeons, suggesting that if vivisections could not be altogether dispensed with, they might at least be deprived of their suffering by the use of anæsthetics. Dr. Delafield referred this letter to Dr. Dalton, who replied that he was in the habit of using anæsthetics in all instances in which it was possible without defeating the object of the experiment; adding that cases of the latter class were extremely rare. This statement was not specific enough to satisfy Mr. Bergh, who wrote another letter, asking information concerning the exceptional cases; which Dr. Dalton explained by stating that they were cases where the object was "to ascertain the presence or absence of sensibility in particular parts."

It seems that these statements were still unsatisfactory, and Mr. Bergh accordingly addressed a long letter to Dr. Delafield, setting forth in the strongest terms his abhorrence of all such practices, giving a detailed description of some of Magendie's experiments, presenting the opinions of prominent members of the medical profession in Europe against them, and concluding with a protest, "in the name of Heaven, public morality and of this Society against these fearful cruelties, inflicted on dumb, unresisting creatures, confided to the merciful protection of mankind, without the employment of anæsthetics." The letter is published in full in the *New York Evening Post* of the 7th inst.

Making all due allowance for the humane feeling which has actuated Mr. Bergh, we feel that in this matter he has gone too far. It is impossible for an unprofessional mind fully to appreciate the considerations which justify to a physiologist what considered by itself merely is but a painful spectacle of suffering. We feel that he should have trusted to the humanity of Dr. Dalton to pursue his researches in the true spirit of scientific inquiry, the sole object of which is the ultimate diminution of human suffering. Least of all was he justified, in our opinion, in printing his letter in the daily press, to excite the universal horror and indignation of sensitive minds. As well might he have opened the doors of a dissecting room to the public for the purpose of producing a prejudice against the indispensable study of practical anatomy. Dr. Dalton replied to Mr. Bergh's letter as follows, and we leave his justification in his own hands; we find it in the *New York Evening Post* of the 8th inst.

COLLEGE OF PHYSICIANS AND SURGEONS,
New York, November 8, 1866.

Edward Delafield, M.D., President of the College of Physicians and Surgeons.

DEAR SIR,—I have received your letter of this date, enclosing a third communication from Mr. Henry Bergh, President of the Society for the Prevention of Cruelty to Animals, dated November 1, in which he enters at length into the question of the propriety of vivisections, and in which he protests, "in the name of Heaven, public morality, and of this Society, against these fearful cruelties, inflicted on dumb, unresisting creatures, confided to the merciful protection of mankind, without the employment of anæsthetics." His letter, I see, is also published in the *New York Evening Post* of yesterday.

Without replying to the extravagant epithets contained in Mr. Bergh's letter, which are of no consequence in themselves, and which will have no weight with those who desire to be correctly informed as to the facts in the case, I desire to mention to you a few considerations which show that the practice of experimenting upon animals for physiological and medical purposes, which is held up to so much animadversion, is an eminently proper one; and, so far from deserving reprobation, is entitled to respect as a valuable and legitimate means of benefiting mankind.

First of all, its sole and ultimate object is the relief of human suffering and the cure of human diseases. It is the best and most valuable means by which our knowledge of physiology is increased; and upon physiology the cultivation and improvement of the whole medical art depend. All intimations that it has any other motive than this—such as display, wantonness, or the indulgence of a reckless cruelty, are false, and do not represent its true character. The knowledge which is obtained by it enables us to understand the natural functions, without which the study of medicine in all its branches would be retarded, and indirectly but certainly the success of medical practice diminished. In point of fact, notwithstanding all unfounded assertions to the contrary, the greatest discoveries in physiology have, in past times, been directly due to its employment; and while we owe to it a large proportion of the useful knowledge which we now possess, it would be the greatest misfortune for medicine and the welfare of mankind, if it were abandoned or neglected in the future.

Secondly. It is not a cruel practice, but may be and is, in the great majority of instances, conducted in a perfectly humane and unobjectionable manner. It can never be the object of the physiologist to inflict unnecessary suffering; but on the contrary, it is desirable both for the sake of humanity and for the attainment of his object to avoid doing so. This is greatly facilitated by the use of anæsthetics. Since the anæsthetic properties of ether and chloroform have been known to us, their employment has been fully as useful to the physiologist as to the surgeon, and they enable us to do the requisite preliminary operations without the animal experiencing any sense of suffering, and even without his consciousness.

As you are aware, it has been my constant practice to employ ether for this purpose, in the College of Physicians and Surgeons, whenever the operation to be performed was calculated to inflict pain upon the animal. This can always be done, except in a very small class of cases, where the aim of the experiment, being to ascertain the existence of sensibility in a particular part, the unconsciousness produced by ether would defeat the object of the operation.

This is especially true in certain experiments of Magendie, performed in 1822, on the roots of the spinal nerves, to which I referred in a former letter, and by which the seat of sensibility in these parts was ascertained. But these experiments are so rare that I have not only never had occasion to do them myself, but do not recollect ever to have seen them performed. Yet I know that they have been done in former times, and have been productive of the most valuable and lasting results; and when requisite for other objects similar experiments might again become necessary and proper, and even here the amount of suffering inflicted is very much less than it is often represented to be. It can only be necessary to obtain an indication of sensibility, and the object of the experiment is accomplished. The practice of performing surgical operations on living animals by students or practitioners as a means of acquiring dexterity, so far as I am aware, is unknown in this country.

I allude to these considerations because I think they are important to the cause of medical education, in which we, as well as the whole community, are interested. In the College of Physicians and Surgeons over which you preside, it has always been believed that the fullest instruction in physiology, as well as in the other elementary branches, is essential to the due preparation of intelligent and successful physicians. The more complete and efficient this instruction is made, the more competent will be the practitioners who every year are sent out to join the ranks of the medical profession. This instruction is made as complete as possible by ocular demonstrations of many important facts, but this is always done, as I believe, in a reasonable and proper way. It is not the case that "revolting barbarities are repeated at lectures for the mere gratification of juvenile curiosity." It is unnecessary to say to those who are familiar with these lectures that no such barbarities are to be seen there; but it may be desirable for the information of those who are unacquainted with the subject, and might otherwise acquire an unfounded prejudice against it. I cannot, therefore, pass over without reply the communication of Mr. Bergh, which is calculated to place in so false a light one of our most valuable means of improvement in medicine.

Yours, very respectfully,

J. C. DALTON, M.D., *Prof. of Physiology.*

The Revue des Cours Scientifiques de la France et de l'Etranger of the 24th of September, published in Paris, contains, as its first article, a translation of Dr. Cotting's Address before the Massachusetts Medical Society, on Disease as a part of the Plan of Creation, by Gaston Garnier.

Dr. A. M. SHEW, lately an assistant physician in the State Lunatic Asylum at Trenton, N. J., has been unanimously elected by the Commissioners of the new Hospital for the Insane in Connecticut Superintendent of that institution.

There are over five hundred matriculants in the Medical Department of the University of Michigan.

VITAL STATISTICS OF BOSTON.

FOR THE WEEK ENDING SATURDAY, NOVEMBER 24th, 1866.

DEATHS.

	Males.	Females.	Total.
Deaths during the week	49	44	93
Ave. mortality of corresponding weeks for ten years, 1855—1865	40.8	36.9	77.7
Average corrected to increased population	00	00	84.26
Death of persons above 90	0	1	1

BOOKS RECEIVED.—Clinical Observations on Functional Nervous Disorders. By C. Handfield Jones, M.B., &c. &c., Physician to St. Mary's Hospital. Philadelphia: Henry C. Lea. 1867.—Surgical Clinic of La Charité. Lessons upon the Diagnosis and Treatment of Surgical Diseases, delivered in the month of August, 1865, by Prof. Velpeau, Membre de l'Institut et de l'Académie Impériale de Médecine. Translated by W. C. B. Fifield, M.D. Boston: James Campbell. 1866.—A Treatise on the Principles and Practice of Medicine, Pathology, &c. By W. Paine, M.D. Philadelphia: University Publishing Society. 1866.

DEATHS IN BOSTON for the week ending Saturday noon, Nov. 24th, 93. Males, 49—Females, 44. Apoplexy, 2—congestion of the brain, 1—disease of the brain, 5—bronchitis, 3—burns, 1—cancer, 1—cholera, 1—consumption, 16—convulsions, 1—croup, 2—debility, 1—diarrhoea, 2—diphtheria, 2—dropsy, 1—dropsy of the brain, 3—dysentery, 2—erysipelas, 1—typhoid fever, 1—typhus fever, 1—gastritis, 2—hemorrhage, 1—malformation of the heart, 1—infantile disease, 4—congestion of the lungs, 2—inflammation of the lungs, 5—necrosis, 1—old age, 4—paralysis, 4—peritonitis, 2—premature birth, 3—scrofula, 1—smallpox, 3—suicide, 1—tonsillitis, 1—thrush, 1—unknown, 8—whooping cough, 1.

Under 5 years of age, 34—between 5 and 20 years, 7—between 20 and 40 years, 15—between 40 and 60 years, 23—above 60 years, 14. Born in the United States, 66—Ireland, 23—other places, 1.